the public at all than that by the the use of the "vague but comprehensible language of ordinary life," such erroneous ideas should be propagated. I can assure Mr. Grant Allen, from no small experience of popular science-teaching, that the public mind is quite capable of drawing a very clear distinction between "living" and "extinct" animals, and would urge him to keep that distinction steadily in view in anything he may hereafter write on the subject.

With reference to the sharks' teeth brought up in the *Challenger* dredgings, in that part of the Pacific between Polynesia and South America, I may mention that only within the last week I have seen a collection of sharks' teeth from a "coprolite" digging in South America, among which was one of five inches by four, closely corresponding in its mineralised conditition with the large *Challenger* teeth; and associated with them were rolled fragments of elephantine molars, presumably *Mastodon Andium*. And yet, according to Mr. Grant Allen, they belonged to our *actual* fauna! W. B. CARPENTER

56, Regent's Park Road, London, N.W., March 20

Fisher's "Earth's Crust"

THE verdict of NATURE on the Rev. O. Fisher's "Physics of the Earth's Crust" is that "One or two points do seem to emerge from this assemblage of calculation as fairly clear and established on tolerably firm foundation. Such as that the contraction of the earth by cooling is inadequate to the production of its greater inequalities." . . That "there must be subterranean irregularities of density." I ask for a fresh trial on the ground that the evidence is in-ufficient.

On the first head, what Mr. Fisher has done is this. He has started with the assumption that no part of the earth became solid till the whole had cooled down to a uniform temperature of 7000° F. With this and some other minor assumptions he has been led to the conclusion stated above. And he has high authority on his side, for this is the assumption made by Sir W. Thomson in his well-known paper, "On the Secular Cooling of the Earth." But the facts that an assumption is not in itself physically impossible, and that it enables you to integrate a tiresome differential equation and obtain numerical results, are not sufficient to establish the truth of the assumption. There are other ways in which the earth may have passed from a fluid to a solid state, some of them, to say the least, quite as probable as that which Sir W. Thomson adopts. I very much fear then that Mr. Fisher cannot be said to have established even his negative proposition. Indeed, to my mind, Mr. Fisher's work seems rather to show that the earth did not consolidate in the way supposed by Sir W. Thomson.

The second point, strongly insisted on not only by Mr. Fisher but by many other eminent physicists, which the reviewer looks upon as finally settled, is the doctrine that the material of the crust must be denser beneath the ocean basins than beneath continents. The belief is grounded on the following argument. If this were not so, the preponderance of land on the northern hemisphere would attract the water, and the consequence would be that the sea-level would be higher in the northern than in the southern hemisphere. The answer is: How do we know that this is not so? At the outside the difference of level would not amount to more than a few hundred feet, and what is there to prove that the mean level of the sea in St. George's Channel is not a few hundred feet farther from the earth's centre than the mean level of the sea at the point diametrically opposite. It might be so, and we should none of us be a bit the wiser. The famous Indian deviation of the plumb-line, too, can hardly be looked upon as conclusive, when we reflect that it has been found capable of explanation in several ways by the ingenuity of the former Astronomer-Royal and the late Archdeacon Pratt. No problem that admits of several solutions can be appealed to as conclusive on a point like this. Mr. Fisher's treatment of the Revelations of the Thermometer cannot either be accepted as satisfactory. Any one who has roughly plotted to scale a section over the St. Gothard sees that a segment of a circular cylinder does not represent, even to a very loose degree of approximation, the contour of the mountain.

The reviewer speaks of the cause to which Mr. Fisher would assign the contortion of the rocks of the earth's crust as hardly adequate; he might have safely gone further. That cause is the injection of lava into fissures, or, in other words, the formation of dykes. That contorted rocks are often traversed by countless dykes is a well-known fact. Take for instance the dykes which seam so thickly the Palæozoic rocks of Scotland; but here the dykes were formed long after the contortions, and besides their general direction does not coincide with the longer axes of the folds into which the rocks have been bent. In other cases of violently-contorted rocks there is a striking absence of dykes; this is so along the coast of Glamorganshire and Pembrokeshire, where we have about the most marked case of intense folding and inversion in the British Isles. And this is still more strikingly the case in that marvellous example of contortion and inversion to bs seen in the Canton of Glaurus, which has been so graphically described by Heim. Nowhere, as far as we know, on the earth's surface has inversion gone to the length it has here; dykes do traverse the Palæozoic rocks, but they none of them run up into the Secondary and Tertiary beds, and the contortion did not begin till towards the end of the Eocene period.

I for one should be only too relieved to think that some certainty, even if it were only of a negative kind, had been arrived at in the problems of the Physics of the Earth's Crust, but I fear we are a long way off this happy consummation at present. A. H. GREEN

Yorkshire College, Leeds, March 14

An Equatorial Solar Spot

THE occurrence of a spot close to the equator is so rare a phenomenon that it may interest some of your readers to know that there is such a spot now on the disc.

On the 6th I noticed a spot, not long entered, and close to the equator; it was a large, well-defined, regular, oval spot, with a mag. axis exactly parallel to the sun's equator. On the 10th, at 10h. 45m. G. Astron. Time, this spot crosssed the prime meridian, at a distance from the centre of the disc equal to 0·120 the radius, measured towards the true north limb, in the direction (156°–336°), *i.e.* parallel to the sun's axis.

This distance, 0·120 R., corresponds to hel. lat. 6°·9, measured from the centre towards the north; therefore, as the latitude of the centre is now 7°·2 south, the true hel. lat. of the spot is 0°·3 S.

The observations for determining the place of the spot were made on the 10th at 4.45, and on the 11th at 12.45; during the interval the spot had crossed the prime meridian, and the "position" of the axis of the spot, which had remained constant from the 6th till 4.45 on the 10th, had, during the interval, changed from (90°–270°), having reference to the sun's axis, to (38°-218°), *i.e.* 52° in 20 hours.

There was no further change from 12.45 to 4.45 on the 11th: and on the 12th the character of the spot was so altered that you could not distinguish any maj. axis at all. The instrument used was the 7½-inch equatoreal refractor. WENTWORTH ERCK

Sherrington, Bray, Co. Wicklow, March 13

Seasonal Order in Colours of Flowers

SIX years ago the question was brought forward in this journal (Vol. xiii. p. 427) whether light has any influence on the colour of flowers. I then called attention to the experiments made by Askenasy in 1875 (*Botanische Zeitung*, 1876, No. 1), from which he inferred that the action of light was different, some flowers being changed by darkness, but others not. Having myself from time to time studied this subject, I have seen, like other observers, that several kinds of pigment appear in complete darkness, but that in many of these cases daylight strengthens the tint and increases the hue. Not only flowers, but also other parts of the plant are thus affected. I found for instance that the shoots of several potatoes grown in the dark were coloured pink, that a bud of an elder-tree formed under the same circumstances two red-coloured internodes, and that crocuses, tulips, and hyacinths produced coloured flowers, whereas *Aucuba japonica* gave red-coloured fruits. It seems from these experiments that the plant is able to produce colouring matter without help from any source of light. But it is an important fact that the colours formed in the dark and those formed in the light often do not possess the same beauty. To prove this, I raised a bulb of hyacinth with two buds (or "noses" as they are called by Dutch florists); one of the two buds was covered by a piece of thick opaque paper to prevent the sun shining upon it, while the other bud was uncovered, and thus could enjoy the sun's influence. After some weeks the difference was very marked, the covered flowers being less intensively coloured than the others. This way of experi-

menting gave more striking results than that proposed by Askenasy, who kept the hyacinths in the dark until the whole inflorescence had opened, then cut off the upper part of the flower-stalk with half of the flowers and exposed it to daylight, while the others remained in their dark place. But he also saw that the latter flowers remained less coloured.

Experiments were also made by me with *Aucuba japonica*, which plant produced white flowers in the dark instead of purplish-brown ones in the light, just as the lilac does when cultivated in winter by florists. On the other hand, flowers of *Crocus vernus* and *Tulipa gesneriana* did not perceptibly change their colour. Among the leaves I experimented upon, I mention a Coleus which in the light produced green leaves with red lines, whereas the same sort gave in the dark yellow leaves with almost colourless lines. Achyranthes behaved very curiously, producing in the dark two normally coloured leaves, but the new-formed internode which supported them was almost white instead of red. This result calls to mind that of Batalin, who found ("Acta horti petropolitani," t. vi.) that plants of *Polygonum Fagopyrum*, which he had raised from seeds in the dark, were quite colourless, so that the difference from those grown in the light is very great, the latter, as is generally known, being of a dark red colour.

It appears from these examples, that those organs which are put into the dark in a very undeveloped state (so that they must much enlarge there) undergo much discolouring, whereas those parts that are hidden in a more advanced stage of growth, lose less colour, in some cases almost none.

Lilacs, for instance, develop in the dark from quite small studs, and so do flowers of Aucuba. Buck-wheat plants grow from small seeds containing a small hypocotyl, that enlarges afterwards to an exceedingly long part. The above-mentioned colourless long internode of Achyranthes came from a very small stud. In all these cases there can be only a small quantity of pigment (or chromogene) in the part before its development, and only this small quantity seems to be spread over the same part when many times enlarged. These cases, accordingly, render it very probable that light is necessary to increase the quantity of pigment, and that the pigment present, originates from the time when the plant was exposed to sunlight.

But tulips, hyacinths, crocuses, berries of Aucuba, &c. lose very little of their colour in the dark. Why is this? Because the buds of the flowers named, when hidden in the bulb are quite complete, and the green fruit of Aucuba had reached nearly their natural size at the time they were deprived of light. I think these flowers and fruits had already stored up a great deal of chromogene which they received from the leaves during the time when the bulb and the fruit were formed, just as they possessed a sufficient quantity of food to reach perfect development of all parts in the dark.

According to this opinion, the colour of a flower, fruit, leaf, &c. when grown in the dark, depends only on the quantity of colour-making matter or chromogene that is contained in the part at the moment when it is withdrawn from the light. This opinion is supported by an observation of Askenasy's (*Bot. Ztg.*, *l.c.*), who found that buds of *Pulmonaria officinalis* in different stages of development, when placed into the dark, got their colours the more perfectly the larger the buds were at the time of darkening. The buds which were smallest at that time exhibited almost white flowers.

My conclusion is then, that light is necessary for forming, if not the colouring matter itself, yet a matter (chromogene) which can easily pass into the pigment. J. C. COSTERUS

Amsterdam, March 12

The Electrical Resistance of Carbon under Pressure

I AM indebted to Mr. Herbert Tomlinson for drawing my attention to his most interesting comparisons between the behaviour of carbon and of metals in respect of change of electric resistance under mechanical stress, and am glad to see that his delicate determinations entirely support my conclusion that the excessively slight change of specific conductivity produced by stress in hard coke carbon cannot possibly explain the great variations of resistance observed in the carbon telephone, carbon rheostat, &c. I have also learned that a similar conclusion was arrived at some time ago by Professors Naccari and Pagliani, and that some experiments made by Prof. W. F. Barrett of Dublin on the buttons of compressed lamp-black prepared in Edison's laboratory for use in his well-known carbon telephone lead to a precisely similar result. I am therefore perfectly willing to admit that before the publication of my experiments this question was virtually settled. It is quite clear that the carbon telephone does not work by any variation in the specific resistance of the carbon, but by the partial opening and closing of the circuit at certain surfaces where the intimacy of the contact can be varied by the vibrations.

Bristol, March 20 SILVANUS P. THOMPSON

Vivisection

As I am named in your article thus headed (p. 429), I shall be obliged if you will permit me a brief explanation of my position in regard to the question. There is doubtless in point of suffering a very great difference between such experiments on animals as those tabulated by Prof. Yeo (and cited in your article) and those common upon the Continent, and of which the horrible tortures perpetrated by Professors Schiff, Mantegazza, and Paul Bert may be taken as fair types. But I fail to see by what means, whether legislative or otherwise, the atrocities of vivisection are to be prevented, while its more moderate practices are to be permitted. Had the sins of the experimentalists been confined to the "hypodermic injection" of a few mice, as Sir James Paget has tried hard to make the public believe, I venture to assert that no outcry would ever have been raised on the subject. I for one should certainly have thought the cause unworthy of support. But the truth is, unfortunately, far otherwise. It is not against the inoculation of "vermin," or the pricking of a "tadpole's tail," that the indignation of the English people has been stirred, but against the prolonged and exquisite torments to which such highly sentient creatures as horses, dogs, and other domestic animals have been and still are subjected, often under the influence, not of chloroform, but of curare. So long as English physiologists continue to cite these things with approval, and openly to regret that the effects of public opinion in this country are such as to throw obstacles in the way of "free" and unlimited vivisection, as practised in the Continental schools, so long, I apprehend, the agitation deprecated in your article, will go on, and gather strength. It is the influence of public opinion only, which in this country finds expression as it does in no other, that has hitherto prevented the physiological laboratory of England from becoming as notorious a scene of horror as that of France, Italy, or Germany. And, let it be borne in mind, that the stir on this subject, which has expressed itself recently in the pages of various reviews and magazines, was set going by the demand of the Medical Congress for *unrestricted* vivisection. Sir William Gull's opening sentence in the *Nineteenth Century* of this month is, therefore, wide of the mark. Not less does he mis-state the case, when, in the course of his concluding observations, he infers that I bring against physiological research the charge of Atheistic tendency. That charge is distinctly made by me, not against legitimate research of any kind, but against a method which I wholly dissociate from "science" properly so called. And it is a charge not lightly made, but based on sound experience and thoughtful observation, unbiassed by "emotion" of any kind.

As it has not been my privilege to escape the customary personal retort of pro-vivisectors with regard to the wearing of fur, feathers, and the utilisation of animals, whether for food, pleasure, or clothing, I shall hold myself indebted to your courtesy for permission to make the following statement in justification of my own consistency:—I never buy furs, feathers, ivory, kid gloves, stuffed birds or other creatures. I have long been engaged in trying to introduce the use of vegetable leather for making boots and shoes, and have devoted much of my time to the question. I detest all "sport" which necessitates the pain and suffering of living creatures, and have written many articles and letters to various journals for several years past against seal-hunting, pigeon-shooting, coursing, battues, and rabbit-gins. Of late years I have added "vivisection" to the list. My husband's horses wear no bearing-reins; and I never see cruelty without interfering at the risk—as I know but too well—of personal insult. Finally, it is twelve years since I tasted flesh or fowl of any kind. ANNA KINGSFORD

11, Chapel Street, Park Lane, W., March 16

[It seems a somewhat unwarranted and unworthy argument that because some foreign experimenters have been guilty of excess, therefore we are totally to suppress in England that which all competent persons know and declare to constitute a most im-